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Renal Vascular Complications Requiring Embolization After Partial Nephrectomy: A Systematic Review and Exploratory Meta-analysis

Ayagiysan Kaneshan

akaneshan@acsom.edu.dm

American Canadian School of Medicine

Monika Nikicicz

Kansas College of Osteopathic Medicine

Asal Gholikhani

Kansas College of Osteopathic Medicine

Ral Vandenhoudt

Yale University

Luis Vera

Desert Regional Medical Center

Gurvan Blackman

American Canadian School of Medicine

David Feldstein

American Canadian School of Medicine

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Abstract

Purpose

To estimate transarterial embolization (TAE) incidence after partial nephrectomy and evaluate surgical-approach associations and treatment outcomes.

Methods

A sensitive literature search across PubMed, CINAHL, and Google Scholar yielded 506 records that underwent manual screening and extraction. Eligible cohorts required identifiable denominators and exact postoperative TAE counts; informative abstracts were eligible. Search dates and complete strategies were unavailable. Patient-denominator proportions and unadjusted within-study risk ratios were pooled using restricted maximum likelihood and modified Hartung–Knapp intervals. Risk of bias used an adapted six-domain framework.

Results

Fifty-four reports were retained: 28 full texts and 26 abstract-only reports. The primary synthesis comprised 52 patient-denominator cohorts, 420 TAE patients, and 17,831 reported patient denominators. Pooled incidence was 2.70% (95% confidence interval 2.23–3.28%; $I^2 = 64.0\%$); full-text-only incidence was 2.93% (2.19–3.92%). Two procedure-denominator reports were excluded from the primary pool. Robotic versus open, robotic versus laparoscopic, and laparoscopic versus open risk ratios were 0.55 (0.11–2.65), 0.34 (0.06–1.85), and 1.92 (0.84–4.40), respectively. Embolization frequently achieved reported hemostasis, sometimes after repeat treatment; renal functional findings varied.

Conclusion

Approximately 3% underwent TAE in reported cohorts, with substantial heterogeneity and no established surgical-approach superiority. Abstract-only evidence, surveillance differences, and possible cohort overlap limit certainty. Radiologists should distinguish imaging-detected lesions from treatment-requiring events and interpret embolization risk alongside clinical presentation and surveillance practices.

Introduction

Partial nephrectomy (PN) preserves renal parenchyma during renal-mass treatment but can be complicated by renal artery pseudoaneurysm (RAP), arteriovenous fistula (AVF), or postoperative hemorrhage. Abdominal radiologists may encounter these lesions during investigation of bleeding or postoperative surveillance, whereas interventional radiologists must distinguish lesions requiring transarterial embolization (TAE) from those managed conservatively. Imaging-detected lesions and treatment-requiring events are therefore different outcomes [1–3].

Selective TAE permits targeted treatment of an injured arterial branch. However, final hemostasis, first-session success, renal-unit preservation, and renal functional preservation are distinct endpoints. Multiple lesions or repeat embolizations in one patient must not inflate patient-level incidence [4–6]. Surgical approach may also be associated with case selection, operative era, tumor complexity, and surveillance rather than independently determine vascular risk [7].

This systematic review and exploratory meta-analysis estimates postoperative TAE incidence in adult PN cohorts, evaluates extractable within-study surgical-approach comparisons, and describes embolization and renal functional outcomes. Its clinical focus is the interpretation of treatment-requiring vascular complications across heterogeneous imaging and surgical practices.

Methods

Design and eligibility

The review analyzed a record set yielded from a highly sensitive literature search on PubMed, CINAHL, and Google Scholar databases and was prospectively registered on PROSPERO. Reporting follows the PRISMA 2020 structure where supported by the available audit trail [8, 9].

Eligible primary cohorts, registries, trials, and series larger than ten required an identifiable PN source denominator and exact postoperative TAE numerator. Reviews, small series, non-English reports, nonseparable pediatric cohorts, radical nephrectomy, transplantation, trauma, biopsy-related injuries, and primary or preoperative tumor embolization were excluded. A separable conventional-PN arm or explicitly defined subgroup was eligible when both counts were clear. Such subgroups were not treated as general PN populations.

Abstracts were eligible when they provided sufficient eligibility information and exact usable counts. Adult eligibility sometimes relied on adult indexing or renal-tumor context because minimum age was unavailable; explicit pediatric signals without separable adult data led to exclusion. Imaging-detected aneurysms, general hemorrhage, and unspecified interventions were not automatically counted as TAE. Unreported events were not converted to zero, and uncertain integers were not reverse-engineered from percentages. Postoperative preventive TAE was retained but distinguished from symptomatic treatment.

Information sources, selection, and extraction

The 506 records were designated from PubMed, CINAHL, and Google Scholar. No duplicate PMID or nonmissing DOI was identified within the export. A comprehensive independent database rerun was performed and yielded no new results.

Retrieval attempts used PMC/Europe PMC, publisher DOI pages, publicly indexed repositories, and targeted journal searches. Remaining unavailable reports were assessed from abstracts; failed full-text

retrieval remained recorded. No author contact or library document delivery was undertaken. Screening and extraction used documented independent duplicate human verification techniques.

Institution, authors, and recruitment periods were compared for overlap. Identifiable companions were not independently pooled. Full-text verification was generally prioritized; among overlapping abstracts, the broader extractable cohort was preferred. Residual overlap remains possible.

Extracted variables included design, period, approach, denominator, TAE count, ascertainment, presentation, imaging, treatment timing and technique, embolic material, technical and clinical success, repeat treatment, surgical rescue, and renal function. Missing or nonseparable outcomes were not imputed. Each treated patient contributed once to incidence; lesions, embolized branches, and repeat sessions remained separate. External referrals were separated from local incidence cohorts. Procedure-denominator reports were excluded from the primary patient-denominator synthesis. The study inventory and counting audit are provided in Online Resource 1.

Risk of bias

The adapted six-domain framework examined sampling/generalizability, cohort coverage, outcome specificity, delayed-event capture, numerical consistency, and approach confounding, informed by JBI resources [10]. Ratings were lower concern, higher concern, unclear, or not assessed/no direct comparison. Missing details were classified as unclear. This was not a validated composite score or a completed ROBINS-I assessment; no formal GRADE assessment was performed. Duplicate independent reviewer assessment used the same framework and produced unchanged final ratings.

Statistical analysis

Study proportions used Wilson 95% confidence intervals. Patient-denominator proportions were pooled on the logit scale using inverse-variance weighting, restricted maximum likelihood between-study variance, and modified Hartung–Knapp intervals with the variance multiplier bounded below by one. A 0.5 correction was applied to each binomial cell only for zero/full-event cohorts. This approximate rare-event method is not a binomial likelihood model.

Within-study unadjusted risk ratios required explicit approach-specific events and denominators. Zero-cell comparisons received a 0.5 correction in all four cells. Log risk ratios were pooled using the same variance and interval framework. Cochran's Q, I^2, and τ^2 described heterogeneity. Forest and funnel plots displayed individual proportions and exploratory small-study effects. Funnel asymmetry was not treated as proof of publication bias; no formal asymmetry test was used [11]. Analyses were implemented in the supplied Python scripts.

Post-extraction sensitivities separated full-text and abstract-only patient cohorts, retained explicit symptomatic or zero-event cohorts, excluded eight reports with population/reporting concerns, and added two procedure-denominator reports in a diagnostic mixed-unit analysis. Full-text-only approach

comparisons were also examined. No formal between-subgroup test was performed. Treatment success and renal function were synthesized narratively because definitions, populations, and observation times were insufficiently comparable.

Results

Selection and study characteristics

Of 506 records, 342 were excluded at title/abstract screening and 164 were pursued for full text. Among 51 retrieved reports, 28 were retained, 18 were ineligible, and five were overlapping companions. Among 113 unavailable full texts, 26 abstracts were retained, 14 were overlapping companions, and 73 were ineligible or insufficiently informative. One additional report identified during retrieval was excluded because its exact TAE numerator remained unclear (Fig. 1).

The final dataset contained 54 reports: 28 retrieved full texts and 26 abstract-only reports. Nineteen companions remained in the audit but were not pooled independently. Fifty-two reports used patient denominators and two used procedure denominators. Reports spanned 2005–2026 and were predominantly retrospective, encompassing general PN cohorts, selected subgroups, technique series, and hereditary/multifocal disease. Patient-denominator cohorts ranged from 15 to 2,076. The complete included-study inventory is in Online Resource 1, Table S1 [1–7, 12–58]. “Abstract only” describes the evidence accessed, not necessarily a conference abstract.

Incidence and surgical comparisons

Across 52 patient-denominator cohorts, 420 patients underwent TAE among 17,831 reported patient denominators. The crude aggregate proportion was 2.36%; pooled incidence was 2.70% (95% CI 2.23–3.28%), with substantial heterogeneity (I^2 = 64.0%; τ^2 = 0.251 on the logit scale). Weighting and between-study variation explain the difference between crude and pooled estimates (Fig. 2).

Sensitivity results are shown in Table 1. Full-text-only incidence was 2.93% (2.19–3.92%) versus 2.47% (1.88–3.22%) for abstract-only reports; similar estimates do not exclude availability bias. The restrictive analysis excluded Li, Larson, Huang, Loffroy, Takayama, Suriano, Leavitt, and Shigeta because of reporting, endpoint, referral, surveillance, subgroup, or overlap concerns. Its estimate was 2.58% (2.11–3.14%). Adding the two procedure-denominator reports yielded 2.76% (2.28–3.35%; I^2 = 74.6%); the mixed aggregate 526/20,253 is not a risk among unique individuals.

Within-study comparisons were unadjusted (Table 2). Robotic versus open, robotic versus laparoscopic, and laparoscopic versus open risk ratios were 0.55 (0.11–2.65), 0.34 (0.06–1.85), and 1.92 (0.84–4.40), respectively. None established approach superiority or equivalence. Full-text-only robotic/laparoscopic comparison involved two studies and had a particularly imprecise interval. Tumor complexity, era, experience, and treatment thresholds could confound all comparisons.

Counting units and clinical outcomes

Morita's 1,166 and Gomella's 1,256 PN denominators represent procedures, potentially repeated within patients [3, 49]. Loffroy's local incidence was 12/298, whereas the treatment series included 25 patients, including 13 external referrals, with 47 RAP lesions, nine AVFs, and 26 embolization sessions [5]. Dividing the referral cohort by the local surgical denominator would overestimate incidence.

Imaging-detected RAP did not always lead to TAE: Takayama reported 28 lesions in 23 patients, of whom four underwent embolization [6]. Morita reported 82 treated patients, 83 sessions, 139 lesions, and 168 embolized branches [3]. These units remained distinct. Morita's 18 urgent and 64 elective treatments were not relabeled symptomatic and asymptomatic. Selected subgroup denominators and conversion handling are detailed in Online Resource 1.

Selective or superselective microcoil embolization was common; other reported agents included gelatin sponge, n-butyl cyanoacrylate, and thrombin. Reported final successes included 29/29 patients in Jeon, 17/17 in Mahmud, 82/82 in Morita, and 25/25 in Loffroy's referral series; the latter three included one repeat treatment each [3–5, 7]. These findings do not establish comparative agent effectiveness or universal first-session success.

Renal functional findings varied. Lee described lower estimated glomerular filtration rate after embolization and during follow-up, and Kahn reported worse recovery among patients treated for RAP; Baboudjian and Chung found no significant between-group difference at their reported time points [2, 37, 38, 55]. These observational comparisons cannot separate effects of embolization from surgery, hemorrhage severity, or baseline renal reserve. Some reports described escalation to renal artery occlusion or nephrectomy [18, 43, 49]. Strobl's abstract printed 35/39 clinical successes and 85.7%; the fraction calculates to 89.7%, an unresolved source discrepancy [29].

Technique reports addressed renorrhaphy, early unclamping, hemostatic adjuncts, selective vascular control, balloon occlusion, and three-dimensional planning. Small selected series and inconsistent endpoints precluded a hierarchy of preventive techniques. Explicit zero events in small cohorts do not establish prevention [40, 45, 53].

Small-study effects and risk of bias

The exploratory funnel plot (Fig. 3) cannot distinguish reporting bias from case mix, surveillance differences, or the mathematical relationship between proportions and their standard errors. Nominal boundaries do not describe the full random-effects distribution.

The adapted appraisal is shown in Fig. 4. Sampling was unclear in 35 reports and of higher concern in 19. Delayed-event capture was unclear in 40 and lower concern in 14. Outcome specificity was lower concern in 52, unclear in one, and higher concern in one; this does not imply low overall bias. Approach confounding was higher concern in all eight assessable comparative reports; 46 had no assessment for that domain. Complete domain ratings and rationales are retained in Online Resource 1.

Discussion

Interpretation for abdominal and interventional radiology

Postoperative TAE incidence was approximately 3% in the reported PN cohorts, but this estimate summarizes heterogeneous practice rather than individual risk. The endpoint combines vascular injury, its detection, and a decision to embolize. More sensitive imaging or a lower preventive-treatment threshold may increase measured incidence without indicating more symptomatic bleeding. Abdominal radiologists interpreting postoperative vascular abnormalities and interventional radiologists evaluating treatment should therefore distinguish lesion detection from clinically consequential bleeding.

Keeping patients, operations, lesions, and sessions separate is clinically consequential. Multiple lesions in one kidney and repeat treatments contribute procedural burden without creating additional patient events. Hereditary or multifocal disease may produce repeated PN procedures within individuals. Referral treatment series may enrich for complex complications and cannot supply numerators for local surgical incidence. These distinctions also matter when comparing institutional outcomes.

Selective TAE frequently achieved reported bleeding control, sometimes after repeat treatment. Final hemostasis nevertheless does not guarantee first-session success, renal-unit preservation, or unchanged renal function. Functional decline may reflect PN, hemorrhage, devascularized parenchyma, baseline renal reserve, or embolization; nonsignificant observational comparisons do not prove absence of harm. Standardized baseline, post-PN/pre-TAE, and subsequent functional measurements would improve interpretation. These data do not establish an optimal embolic agent or support routine preventive TAE for every imaging-detected lesion.

Surgical comparisons and limitations

Unadjusted comparisons did not demonstrate a conclusive difference between robotic, laparoscopic, and open approaches. Wide intervals and sparse events permit clinically meaningful differences in either direction. Nonrandom allocation, operative era, tumor complexity, and center experience preclude a causal ranking. Technique-specific findings remain hypotheses for adjusted comparative research.

Nearly half the reports were assessed from abstracts, limiting verification of age, exclusions, follow-up, and outside-hospital treatment. Adult renal-tumor context sometimes informed eligibility. Selection on an explicit TAE count could favor complication-focused studies, whereas incomplete delayed-event capture or RAP-only endpoints could underestimate treatment frequency. Similar full-text and abstract-only estimates cannot eliminate these biases. Residual institutional or registry overlap may inflate precision; summed denominators are not verified unique individuals.

Sensitivities were selected after extraction. Approximate logit variances and continuity corrections may behave differently from binomial likelihood models for rare outcomes [59]. Heterogeneity persists despite random-effects pooling, and inconsistent treatment-outcome definitions prevented defensible pooled efficacy or renal-function estimates.

Clinical implications

Future work should report unique patients, PN procedures, vascular lesions, treated patients, and TAE sessions separately, distinguish symptomatic from imaging-driven treatment, and capture delayed events. Surgical comparisons require adjustment for tumor complexity, renal reserve, era, and center or surgeon effects. For abdominal radiologists and interventional radiologists, the practical message is to distinguish detected vascular lesions from treatment-requiring events and interpret embolization risk alongside symptoms, surveillance, and patient-specific renal reserve, without assuming superiority of a surgical approach.

Declarations

Author Contribution

A.K. : conceptualization, investigation, data curation, writing - original draft, project administration. M.N. : investigation, data curation, writing - review and editing. A.G. : investigation, data curation, writing - review and editing. R.V. : methodology, formal analysis, visualization, writing - review and editing. L.V. : supervision, clinical interpretation, writing - review and editing. G.B. : supervision, clinical interpretation, writing - review and editing. D.F. : supervision, clinical interpretation, writing - review and editing.

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Tables

Table 1 Incidence and sensitivity analyses

Analysis	Reports	TAE / denominator	Pooled %, 95% CI	I^2
Primary patient cohorts	52	420/17831	2.70 (2.23–3.28)	64.0%
Full-text patient cohorts	27	214/8558	2.93 (2.19–3.92)	69.4%
Abstract-only patient cohorts	25	206/9273	2.47 (1.88–3.22)	52.4%
Restrictive patient cohort subset	44	359/15125	2.58 (2.11–3.14)	57.7%
Mixed patient/procedure units	54	526/20253	2.76 (2.28–3.35)	74.6%
Explicit symptomatic / zero	34	252/10226	2.83 (2.21–3.62)	64.0%

Values are pooled percentages with 95% confidence intervals. TAE, transarterial embolization. Mixed patient/procedure units are diagnostic only and must not be interpreted as unique patients. All sensitivity restrictions were selected after extraction.

Table 2 Unadjusted within-study surgical-approach comparisons

Evidence	Comparison	Studies	RR, 95% CI
Full text + eligible abstracts	Robotic vs open	4	0.55 (95% CI 0.11–2.65)
Full text + eligible abstracts	Robotic vs laparoscopic	3	0.34 (95% CI 0.06–1.85)
Full text + eligible abstracts	Laparoscopic vs open	6	1.92 (95% CI 0.84–4.40)
Full text only	Robotic vs open	3	0.30 (95% CI 0.06–1.51)
Full text only	Robotic vs laparoscopic	2	0.36 (95% CI 0.0002–616.44)
Full text only	Laparoscopic vs open	4	1.16 (95% CI 0.51–2.65)

RR, risk ratio; CI, confidence interval. Estimates use log risk ratios, restricted maximum likelihood, and modified Hartung–Knapp intervals. Comparisons are observational and unadjusted; confidence intervals do not establish equivalence or causal superiority.

Figures

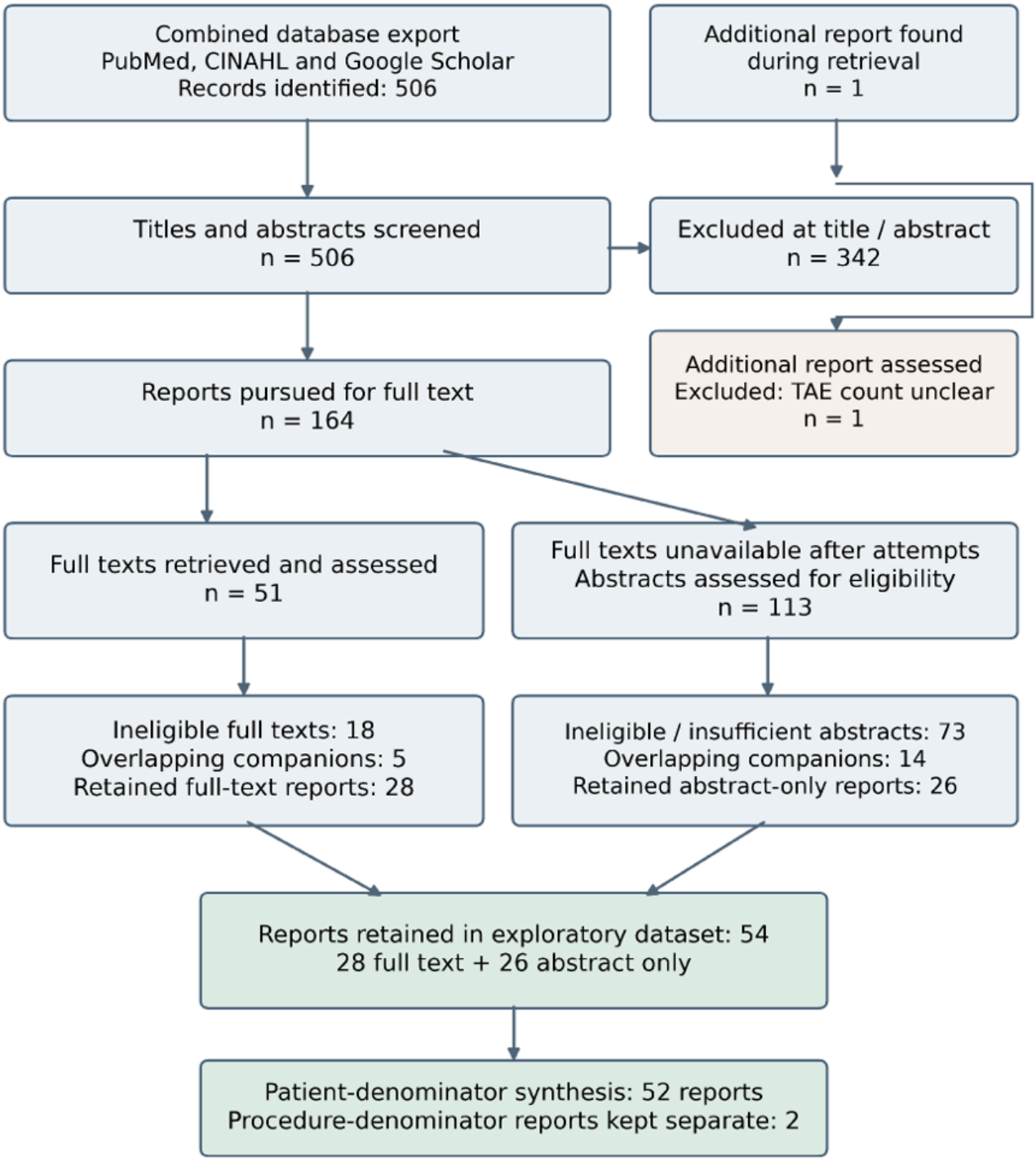

Figure 1

Adapted study-selection flow of the records yielded from the sensitive literature search representing PubMed, CINAHL, and Google Scholar. Unavailable full texts could contribute eligible abstract data. Nineteen companions remained in the audit but were not independently pooled. The additional retrieval report was excluded because its transarterial embolization (TAE) count was unclear.

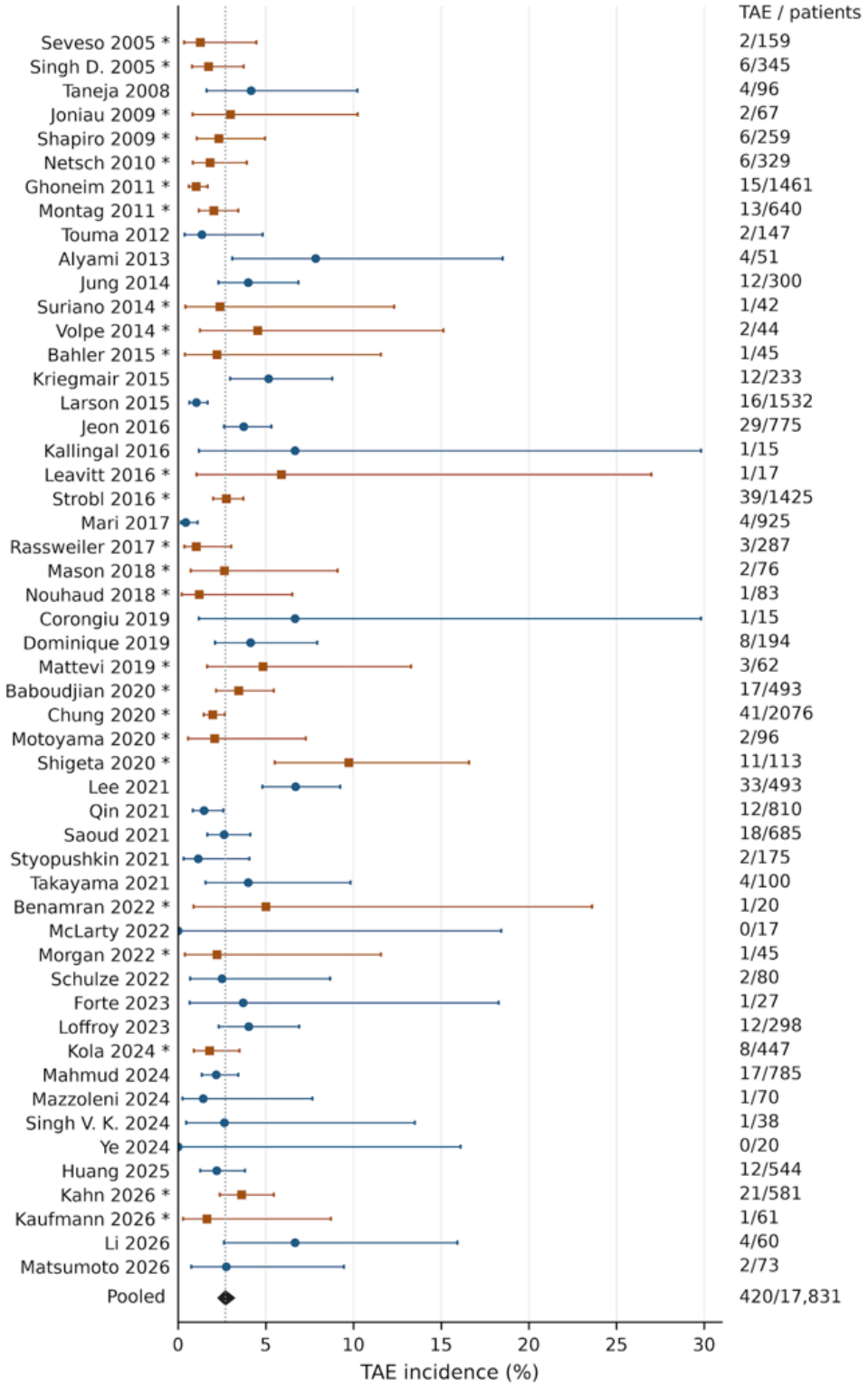

Figure 2

Postoperative transarterial embolization (TAE) incidence in 52 patient-denominator cohorts. Whiskers show Wilson 95% confidence intervals; the diamond shows the pooled estimate, 2.70% (95% confidence interval 2.23–3.28%), from the inverse-variance logit restricted maximum likelihood model with modified Hartung–Knapp intervals (I^2 = 64.0%; τ^2 = 0.251). Squares and asterisks identify abstract-only reports;

circles identify retrieved full texts. The dotted line marks the pooled incidence. Procedure-denominator reports are excluded.

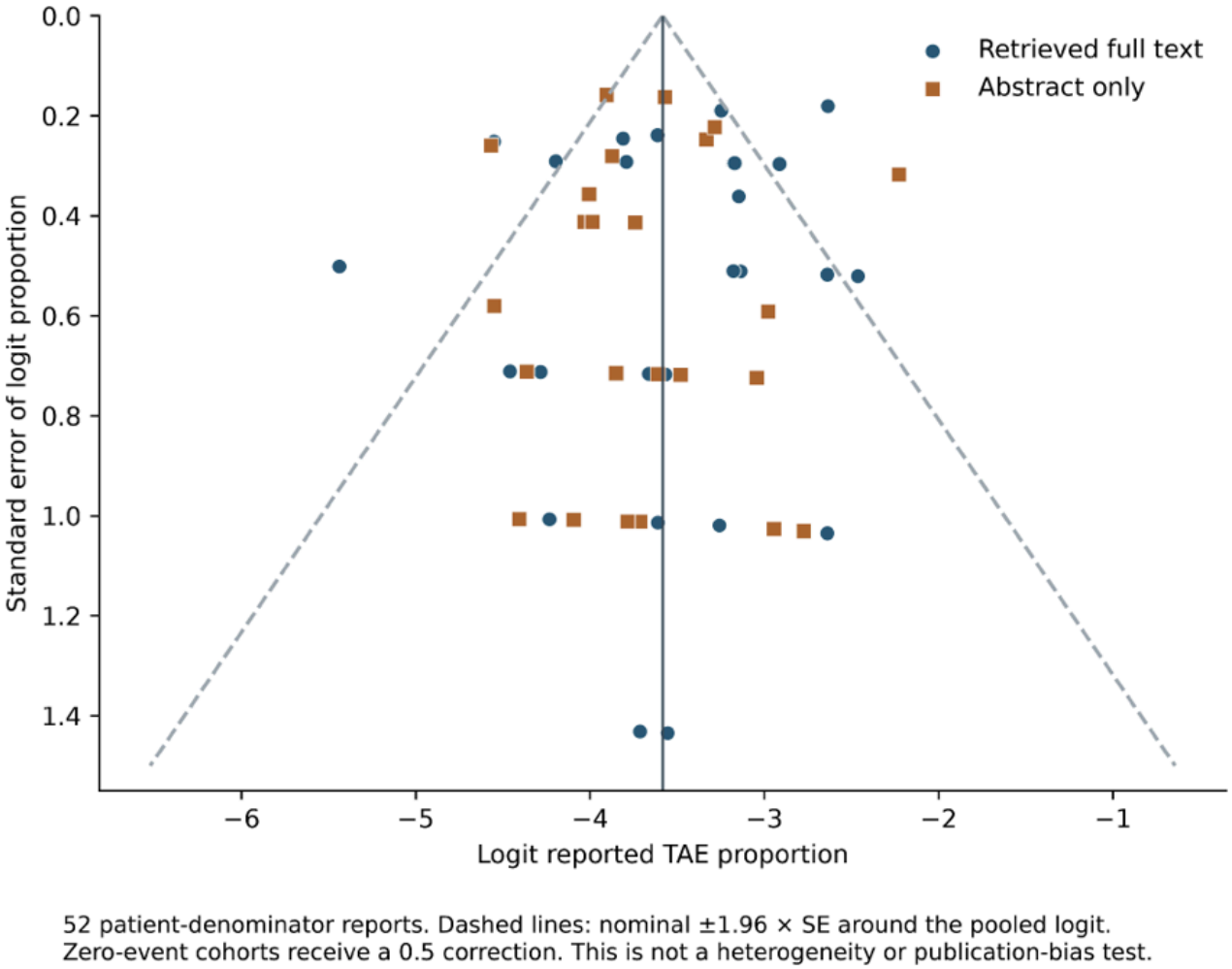

Figure 3

Exploratory funnel plot of 52 patient-denominator cohorts. The horizontal axis is the logit of the reported transarterial embolization proportion and the vertical axis its standard error. Zero-event cohorts receive a 0.5 cell correction. The solid line marks the pooled logit; dashed boundaries are nominal ±1.96 standard errors and do not incorporate all between-study heterogeneity. The display does not establish the presence or absence of publication bias

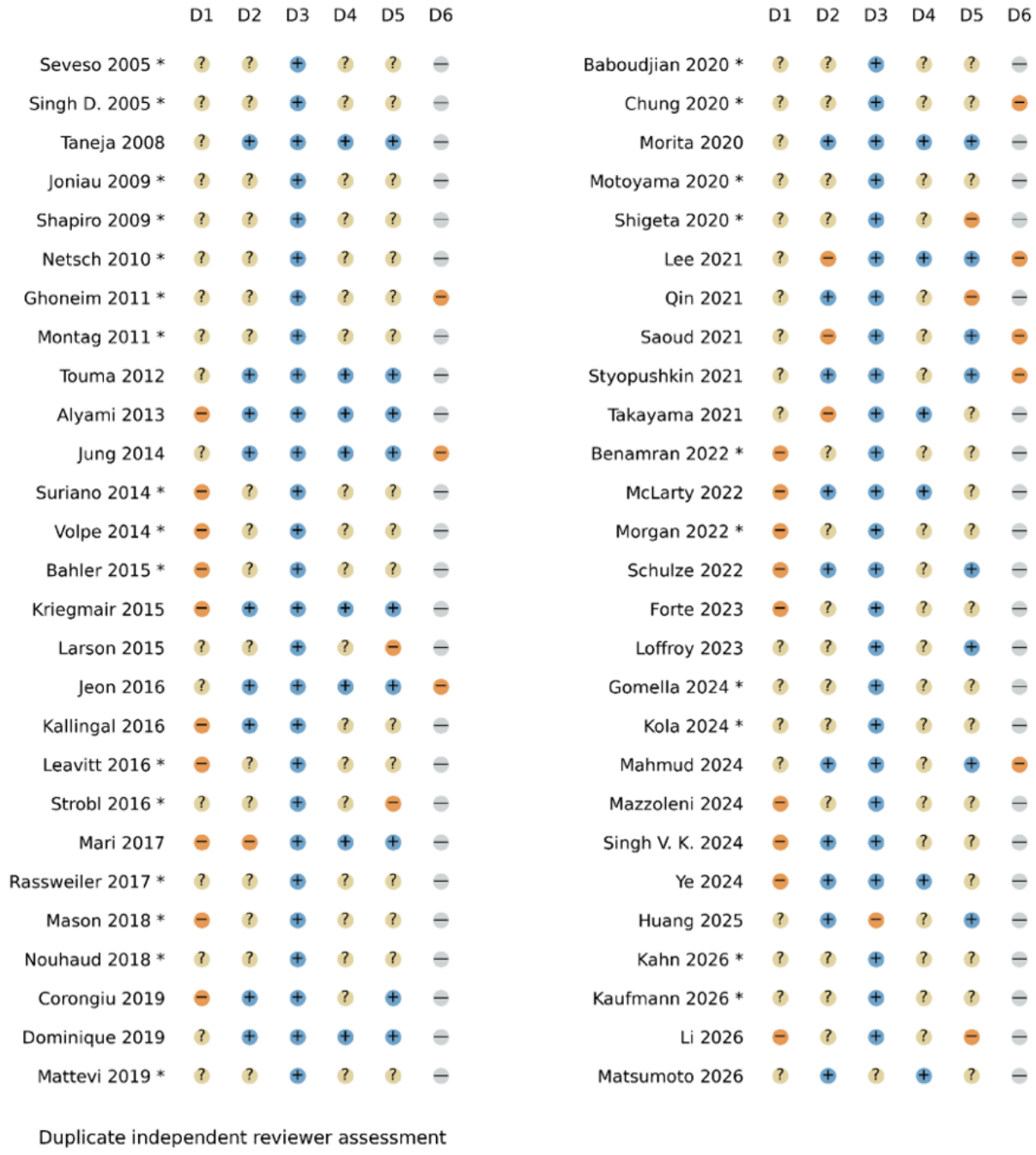

Figure 4

Adapted six-domain appraisal of 54 reports: duplicate independent reviewer assessment. Final ratings were unchanged from the initial AI-assisted appraisal. D1, sampling/generalizability; D2, cohort coverage; D3, outcome specificity; D4, delayed-event capture; D5, numerical consistency; D6, approach confounding. Symbols denote lower concern (+), higher concern (−), unclear (?), and not assessed/no direct comparison (—). Asterisks identify abstract-only evidence. No validated overall score or formal certainty rating was assigned

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- IRRenalSurgerySupplementaryMaterial.pdf